MOTHER TONGUE

Newsletter of the ASSOCIATION for the STUDY of LANGUAGE IN PREHISTORY.

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CHANGES OF ADDRESS & NEW HANDS AT THE HELM

A number of stable features of ASLIP will now change. For one thing the present Rockport address is now ONLY our legal address. At some time in the next year it will cease even to be our legal address. Our mailing address changes forthwith. If one wishes to write to the person in charge of ASLIP for the coming year or the editor of MOTHER TONGUE for the November issue, then write to the Vice-President:

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TADDESE BEYENE

Do not forget that your letters are the life blood of ASLIP !

GÖTTINGER MISZELLEN

Beiträge zur ägyptologischen Diskussion

Heft 109

Göttingen 1989

Unser Kopf ist rund, damit das Denken die Richtung wechseln kann.

F. PICABIA

Peter Behrens

*19.7.1931 in Bremen   †11.2.1989 bei El Ayat/Ägypten

Im Sommersemester 1970 begann Peter Behrens, knapp vierzig, das Studium der Ägyptologie in Köln. Er war ein außergewöhnlicher Student, den außergewöhnliche Gründe zu dieser Wissenschaft geführt hatten: er wollte ein Fach studieren, bei dessen Abschluß nicht ein bestimmter Beruf entstünde, ein Fach, das nicht primär eine Ausbildung zum Geldverdienen böte, sondern Lehrstoff nur als Grundlage für dessen Überprüfung, als Ausgangsbasis für Forschung vermittele.

Dieser Wunsch entsprang früheren Erfahrungen. Er erzählte von seiner Lehre im Bergbau, wo er von dem besonderen Menschenschlag unter Tage sehr beeindruckt gewesen war. In einigen Semestern an verschiedenen Universitäten hatten ihn die Fächer Jura und Medizin mit ihren Inhalten fasziniert, die Berufsaussichten - einerseits Termine und Prozeßakten, andererseits Krankenhäuser und Arzt-"Aura" - jedoch abgeschreckt.

Im Jahr seiner Zwischenprüfung, 1972, erschien die deutsche Ausgabe von C. ALDRED, *Die Juwelen der Pharaonen*, aus dem Englischen übersetzt

von Peter Behrens. Von diesem "ausgebildeten" Talent hatte er bisher nicht gesprochen, obwohl er damit sehr erfolgreich gewesen war: jahrelang hatte er amerikanische Prosa, die teilweise für unübersetzbar galt, ins Deutsche übertragen.

Doch nun saß er im Seminar, zunächst als Student mit Nebenjobs, dann als studentische Hilfskraft, später als "Sekretär". Sein Schreibtisch war bal ein Angelpunkt für Studenten und Studentinnen, Kollegen und Professoren: wissenschaftlich und menschlich war er stets zu einem Gespräch bereit, sein Rat, seine Unterstützung und auch seine Kritik fanden wohl in jeder Arbeit, die in der Bibliothek entstand, ihren Niederschlag. Das Leben im Seminar wurde von ihm nachhaltig geprägt. Übungen für Studenten, Kurse und Vorträge an Volkshochschulen, aktive Teilnahme an Kongressen im In- und Ausland sowie acht Jahre lang mehrere Reiseleitungen pro Jahr machten ihn als Ägyptologen vielleicht bekannter als seine Schriften es bisher vermochten.

Bis zum Magister war sein Themengebiet vornehmlich das Mittlere Reich. Die Särge aus der mittelägyptischen Nekropole von El Berscheh unterzog er einer minutiösen Analyse: anhand der variierenden Schreibungen des Determinativs des Wortes 'Maat' gelangte er zu einer relativen Chronologie der Särge.

Nicht Memphis und Theben, sondern Libyen, die ägyptischen Provinzen und Nubien waren später die kulturellen Gegenden, denen er seine Forschungen widmete. Lokalen Göttern, Trachten und Bräuchen der ägyptischen Frühzeit war er auf der Spur, wobei er oft afrikanische Nachbarn als Vergleich heranzog. In diese Zeit fiel seine Mitarbeit am LEXIKON DER ÄGYPTOLOGIE. Stichworte wie *Nacktheit, Phallus, Phallustasche, Pfeil* oder *Uch* lassen dieses Interesse erkennen. Die Tierthemen *Geflügel (-hof, -zucht), Kalb, Skorpion, Stierkampf, Strauß*

(-enei, - enfeder) und *Widder* erscheinen auf den ersten Blick teilweise nicht besonders aufsehenerregend, doch ergaben sich für ihn bei näherer Betrachtung stets neue Aspekte und Alternativen gegenüber dem bisher Geglaubten. Die spärlich bemessenen Anmerkungen hätten oft zu längeren Aufsätzen ausgebaut werden können, wenn er nicht in ständigem Kampf mit der stets zu schnell verrinnenden Zeit gestanden hätte. In den Stichworten *Hockerbestattung, Nomaden (und Bauern)* sowie *Reichseinigung* spiegelte sich die Hauptfrage seiner zukünftigen Forschung, die auch in der Wahl seiner Nebenfächer, Afrikanistik und Ur- und Frühgeschichte, erkennbar war: Welche Rolle spielte der "afrikanische" Kontext bei der Entstehung der "ägyptischen" Kultur?

Eine eingehende Untersuchung widmete er dem Vergleich des Viehzuchtvokabulars der Ägypter mit dem seiner südlichen und westlichen Nachbarn. Voraussetzung für jeden phonologischen Vergleich war für ihn dabei, daß die ethnologisch bzw. archäologisch belegte außersprachliche Wirklichkeit in den herangezogenen Kulturen weitestgehend übereinstimmte. Diese sehr strenge Methode war ergiebig, und es entstand langsam eine Abhandlung über "Wanderungsbewegungen und Sprache der frühen saharanischen Viehzüchter". Erst unter eindringlicher Aufforderung seiner Lehrer und Kollegen war er bereit, sie als Dissertation einzureichen.

Es begann eine umfangreiche Sammelarbeit, deren Auswertung dazu führen sollte, die Stellung des Ägyptischen im Rahmen der afroasiatischen Sprachfamilie zu bestimmen und seine Phonologie auf Grund dieser Bezüge genauer zu rekonstruieren, als es bislang möglich ist. Als nicht minder interessantes Nebenprodukt wären dadurch etliche Kulturzüge der Frühzeit Ägyptens ablesbar geworden. Seine Arbeitshypothese, die er mit einer wachsenden Zahl von Forschern dieses Gebietes teilte, war, daß das Diffusionszentrum der verschiedenen afroasiatischen Sprachen (Semitisch, Ägyptisch, Berber, Kuschitisch, Omotisch und Tschadisch) in Nordostafrika

gelegen habe und die alten Kulturen des östlichen Mittelmeerraumes nicht dem Zweistromland entstammten, sondern dem Oberen Niltal, im Gebiet des heutigen Sudan. Da das Ägyptische bisher nur unzureichend mit Berber- und Nilsprachen verglichen worden ist, zeichnete sich ab, daß seine Untersuchungen diese bahnbrechende Theorie stark untermauerten. Er wußte aber auch, daß damit den gewohnten Vorstellungen und Ideologien bezüglich des "Fruchtbaren Halbmonds" als "Wiege der Zivilisation" weiterer Boden entzogen werden würde.

Seine Rekonstruktionen waren interessant, die ersten Ergebnisse überzeugend und verblüffend. Von den unzähligen Zetteln und Mappen, die er hinterlassen hat, hat er bislang nur einen winzigen Ausschnitt preisgegeben: die ersten Äußerungen in dieser Richtung sollten unanfechtbare Beweise und von allen Seiten abgesichert sein. Perfektionistisch und sensibel, wie er war, ängstigte er sich vor dem kleinsten Fehler. Trotz der begeisterten Zustimmung vieler Kollegen blieb er oft lange Zeit fassungslos gegenüber den manchmal brutalen und unfairen Angriffen aus dem Lager der Konservativen.

Nachdem er im Januar dieses Jahres endlich zum ersten Mal in den Sudan gereist war und in Khartum mit einem Vortrag bei der 6. Internationalen Meroitisten-Konferenz noch einmal großen Erfolg gehabt hatte, erlag er nun, am letzten Tag einer Reiseleitung, auf der Fahrt ins Fayum einem Herzversagen.

Die besondere Persönlichkeit von Peter Behrens hat - nicht nur in Köln - eine Bereitschaft zum Zweifel an gängigen Vorstellungen geweckt, die sicherlich weitere Forschungen prägen wird.

Philippe Derchain und Ursula Verhoeven-van Elsbergen

SCHRIFTENVERZEICHNIS

A Wissenschaftlich

1) unveröffentlicht

Untersuchungen zur Paläographie einiger Särge des Mittleren Reiches, Köln 1979
(Magisterarbeit)

2) Aufsätze

*C-Group-Sprache - Tu Bedawiye - Nubisch, Ein sprachliches Sequenzmodell und seine
geschichtlichen Implikationen,* in: SUGIA (Sprache und Geschichte in Afrika) 3,
Hamburg 1981, 17-49

Sinuhe B 134 ff oder die Psychologie eines Zweikampfes, in: GM 44, 1981, 7-11

Das afroasiatische Diminutivmorphem t im Ägyptischen, in: GM 57, 1982, 17-24

Wanderungsbewegungen und Sprache der frühen saharanischen Viehzüchter, in: SUGIA 6,
1984/85, 135-216
(bearbeitete Fassung der
Dissertation)

mit M. BECHHAUS-GERST:
"Libyans" - "Nubians". Mutations of an ethnonym, in: AAP (Afrikanistische Arbeitspapiere)
4, Köln 1985, 67-74

*Language and migrations of the early Saharan cattle herders: The formation of the Berber
branch ,* in: Libya antica, Report and
papers of the Symposion
organized by theUNESCO in
Paris 16th to 18th January
1984, UNESCO 1986

The "Noba" of Nubia and the "Noba" of the Ezana inscription: a matter of confusion (Part I),
in: AAP 8, 1986, 117-126

3) Rezensionen

mit PH. DERCHAIN:
L.H. LESKO, *Index of the Spells on Egyptian Middle Kingdom Coffins and Related
Documents* (Berkeley 1979), in: OLZ 78, 1983, 449-450

W. VYCICHL, *Dictionnaire étymologique de la langue Copte* (Leuven 1983), in: Enchoria 15,
1987, 237-245

4) Ausstellungs- und Kongressnotizen

Wat jlänz is Jold (Bemerkungen zur Tutanchamun-Ausstellung in Köln), in: StadtRevue
5.Jg., Nr. 17, Köln August
1980

mit PH. DERCHAIN, H.J. THISSEN, U. VERHOEVEN:
Wider den Geist der SÄK? Offener Brief an die "Ständige Ägyptologen-Konferenz" ,
in: GM 61, 1983, 7-8

5) Lexikonartikel

Stichworte im *Lexikon der Ägyptologie (LÄ),* 6 Bde., Wiesbaden 1975-1986:

Geflügel	LÄ II, 1977, 503-505
Geflügelhof	LÄ II, 1977, 505-507
Geflügelzucht	LÄ II, 1977, 507-508
Hemaka	LÄ II, 1977, 1115-1116
Hockerbestattung	LÄ II, 1977, 1227-1228
Kalb	LÄ III, 1980, 296-297
Nacktheit	LÄ IV, 1982, 292-294
Nomaden (und Bauern)	LÄ IV, 1982, 522-524
Pfeil	LÄ IV, 1982, 1005-1007
Pfeile, Aussenden der	LÄ IV, 1982, 1007-1008
Phallus	LÄ IV, 1982, 1018-1020
Phallustasche	LÄ IV, 1982, 1020-1021
Reichseinigung	LÄ V, 1984, 208-211
Riegel	LÄ V, 1984, 256-257
Skorpion	LÄ V, 1984, 987-989
Stierkampf	LÄ VI, 1986, 16-17
Strauß	LÄ VI, 1986, 72-75
Straußenei	LÄ VI, 1986, 75-77
Straußenfeder	LÄ VI, 1986, 77-82
Uch	LÄ VI, 1986, 820-821
Widder	LÄ VI, 1986, 1243-1245

B Aus dem amerikanischen Englisch übersetzt

1) Belletristik

W.S. BURROUGHS	*The Naked Lunch*	Limes	1962
W.S. BURROUGHS	*Junkie*	Limes	1963
K. PATCHEN	*Erinnerungen eines schüchternen Pornographen*	Limes	1964
W.S. BURROUGHS/ A. GINSBERG	*Auf der Suche nach Yage*	Limes	1964
E. MCBAIN	*Die Axt*	Ullstein	1965
T. VEITCH	*Die Luis Armed Story*	Kiepenheuer&Witsch	1970
W.S. BURROUGHS	*Nova Express*	Limes	1970
W.S. BURROUGHS	*Soft Machine*	Kiepenheuer&Witsch	1971
R. COOVER	*Die öffentliche Verbrennung*	Luchterhand	1983

2) Mitarbeit an Anthologien

E. REAVIS	*Rauschgiftesser erzählen*	Bärmeier&Nikel	1967
R.D. BRINKMANN/ R.R. RYGULLA	*Acid*	März	1969
R.D. BRINKMANN	*Silverscreen*	Kiepenheuer&Witsch	1971
R.E. JOHN	*Mondstrip*	März	1971
D. ODIER	*Der Job - Interviews mit W.S. Burroughs*	Kiepenheuer&Witsch	1973

3) Sachbücher

A.C. CLARKE	*Unsere Zukunft im Weltall*	Lübbe	1969
C. ALDRED	*Die Juwelen der Pharaonen*	Praeger	1972

C Literarisch

Veduten - 33 Tableaus

(entstanden 1973-1983)

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in: Literarische Paradiesvögel, hg. von StadtRevue und Der Andere Buchladen, Köln 1984

in: Gläserne Herzen, Kölner Liebesleben literarisch, hg. von E. PFEIFER und H. DETERS, Köln 1988, 22-25

This obituary was written by Peter's colleagues at Köln and kindly sent to MT by Wolfgang schenkel.

I regret that time has not permitted an English translation. However, Peter and I might agree that a very fine tribute to him would sound right and proper in German. Alas, we have lost a very fine colleague!

Hal

Comments on the Nostratic reconstructionsx of Illič-Svityč (as translated by Mark Kaiser)
by A. Murtonen

Apart from some general observations, comments are generally concentrated on the 'AA.' (or Semito-Hamitic) entries.

3. /bq-/ 'to look' appears abstracted from secondafy meanings of triradical roots such as /bqV/ (originally 'to stay'), /bqr/ ('to split', as also /bq&/), /bqš/ ('to seek'); moreover, /bqV/ is attested in WSem and /bqš/ in NWSem onlyx, hence their pre-Sem origin is very unlikely, cf. n. 70 to the introduction of Hebrew in its West Semitic setting {xxxxxxxx Part I Section Bb (to appear by the end of 1989) and in greater detail in the forthcomming Part III §35 (with a large footnote).

4. 'AA.' entry appears tenable, but support only from Alt. withk irregular /g/ for /&/ correspondence, connection must be deemed doubtful.

5. Appears tenable.

6. Again, supported by Alt. only, and even for 'AA.', the basis is narrow.

8a. Again, /br-/ 'grab, catch' appears abstracted from rather narrowly attested trirad. roots; most original meaning of /br/ appears to be 'pure, clear'.

9. The basic meaning of /brg/ appears to be 'to ascend' rather than 'high'; generally, adjectival meanings are secondary and/or rather late.

10. Cf. 6 above.

11 (14.17 etc.). Onomatopoeic words being imitative of natural sounds could have more than one independent originx; unusual phonetic correspondences would corroborate this, but even in the absence of these, their relevance must be deemed doubtful.

12. Again abstracted from trirad. roots of which /bcl/ only attested outside Sem, and as this is represented primarily by a *Kulturwort* which spread from language to language with the designated objects, the relevance of the 'AA.' entry is doubly doubtful.

13. The 'AA.' entry appears abstracted from two different roots, /bl(l)/ which means basically mixture involving both dryx and wet ingredients; and /b&l/ in a highly specialized secondary meaning derived from the name of an atmospheric deity (with secondary loss of the pharyngal).

16. /'bl/ appears to be of secondary origin, based on birad. /bl/, cf. /wbl/ = /ybl/, /nbl/, /tbl/, /blV/, ?/bwl/, all concerned with vegetation or growth in general, butx often also in the negative sense of withering, aging or wearing out; the primary reference may thus have been to a meadow or steppe in a collective sense, growing and withering seasonally.

20. Cf. 13 above (on /bl(l)/).

21. Appears valid.

22. The primary reference appears to be to uncultivated wasteltandx as this usually involved sand, the next item may be related, if relevant.

23. See 22 above.

24. See 11 above; moreover, the 'AA.' basis appears flimsy.

28. Basic 'AA.' reference appears to be to diseased swelling or growth or secretion, hence hardly comparable to sufficiency.

30. /bnṭ/ attested in two infrequent *Kulturwörter* only (cf. 12 above).

31. Meaning 'knee' appears secondary (primary fertilization).

32. /r/ secondary sound (primary /n/).

33. /sl-/ again abstracted from trirad. roots; moreover, meaning 'split, cut, point' rather weakly represented even in them; the basic meaning of /sl(1)/ appears to be 'to ascend'; /s/ is not a primary Sem-Ham sound either.

3𝔁6. /str/ is a secondary root, formed from /swr/ by /t/-infix.

37. /cx(x)/ is basically biṭad., the semivowel augments clearly secondary where found; IE. Ural. Drav. entries also otherwise hard to reconcile.

38. Onomatopoeic! (cf. 11 above); /-x/ hardly conceivable as a secondary augment either; /cwV/ would have been semantically also closer, but even it may be onomatopoeic.

40. /s(w)r/ in a relevant sense is unknown to me except as a var. of /šor/ = θωρ = taurus, a *Kulturwort* of unknown origin; /cwr/ too mostly refers to a𝔁 herd of *domesticated* animals and may be related to the meaning, 'to besiege' in the sense of being confined to a cattle enclosure and partly spread as a *Kulturwoort* again.

42. /s(w/y)n/ 'to know' has limited attesxtation and the origin of /s/ is un- clear.

44. Again, /cl-/ 'to jump' can only have been abstracted from materials of patchy distribution and uncertain relevance.

50. /θ(y)r/ 'faeces' is unknown to me, but can hardly be related tox𝔁x/θr/ referring to moisture; moreover, /θ/ as an independent phoneme is late, not even common Sem.

51. Cf. 50 (end) above.

53. Ditto.

54. /cm/ 'bitter' is patchily attested, 'sour' does not mean the same, and 'astringent' is again abstracted from trxirad. roots; /sam/ 𝔁 'poison' is a *Kulturwort*.

56. Cf. 51 above; there is no positive evidenxce for interchange of /θ/ with /c/ in genuine Sem roots either.

57. Ditto; not with /ṭ/ either.

59. /d/-/δ/-/z/ primarily deictic rather than locative.

60. /d/ 'and' probably derived from /('/y)d/ 'with, sidde, hand'.

61. /dq/ 'nearby𝔁' is unknown to me; basic meaning of the root is 'be minute, fine (ground/flattened etc.).

62. Primary meaning of /dlx/ appears to be 'to muddy (water)' and it never re- fers to waves or sea.

63. Again, it is hard for me to find even trirad. roots from which /dm-/ 'cover, close, press' could be abstracted; the basic meaning of /dm(m)/ may be silence or faint noises, with subdued activity.

67. Could be valid, although the basic meaning of the root seems to be production of abundant offspring or crop.

73. Cf. 63 above.

74. Again, I find it hard to identify the source of /d(w)r/ 'deaf'; the basic meaning of /dwr/ is circular movement.

75. Equally hard; and with only /d-/ in common with the other entries, hardly valid.

76. Could be valid, althouggh attested in Sem onxly.

81. The primary meaning of /gīd/ appears to be 'nerve, sinew'; again attested in Sem only.

82. /ghr/ has no specific reference to sun or day, but mainly to intense light anc or heat and its effects.

84. The primary meaning of /glx/ appears to be 'to shave', without reference to smoothness or shining.

87. x/gwp/ seems to be secondary/of expansion /gw/ and refer primarily to body interior from which the adjectival 'hollow' (not 'empty') is a further abstraction.

88. Root /gw&r/ is unknown to me; /g&r/ and its (Eth) var. /g^{w}r&/ is onomatopoeic, meaning rough and loud noises.

90. /g(w)rH/ again unknown to me, but conceivable as an extension of /gūr/ which, however, means primarily a young carnivore and appears to be a wandering word.

91. Admittedlty onomatopoeic (cf. on 11 above).

92. Could be valid.

94. Ditto, though possibly onomatopoeic.

95. /g(w)r/ root var. of /ghr/, cf. 82 above.

98. /ɣwr/ refers to depression in the ground rather than 'deep water'; appears differentiated from /&wr/ in WSem only.

99. /ɣm(y)/ differentiated from /&mV/ in Arab (SAr?) only.

100. Could be valid, though root mostly /'wV/.

101. Ditto, though root var. /xwV/ could be more original (/w/ vs. /y/ originally allophonic).

111. Tense differentiation secondary and late.

121. Normally /hV/, but with sibilant var. which appears more original; hence hardly relevant.

122. Connected with 121 above.

123. /'alyat/ refers primarily to the fat tail of a kind of sheep (not used as food).

124. /'mr/ probably originally birad., related to /ml(1)/; no reference to day.

125. Cf. 11 above.

126. /&rb/ (not /'rb/) has no reference to witchcraft, and /-b/ could be secondary, cf. /&rm/, /&rp/ semantically close.

127. Semantically not very close, and possibly onomatopoeic.

128. Phonetically and semantically possible, but with only one firm consonant, syntactically deviating and geographically remote hardly probable.

129. Cf. 128 above; also phonetically vague.

130. Ditto; also scantily attested.

131. Ditto.

132. Verbal attestation secondary or doubtful; mainly used as existential particle, 'there is...'

133. Unknown to me (denominative from /'ammat/ 'forearm'?).

134. Pronominal only as a secondary var.; verbal preform. probably from vocative/ deictic /yā/.

135. Wandering word.

136. Reflexive stem of the root /'wV/.

137. Reference to mountain rarely attested, evidently secondary.

138. /&ubb/ 'lap, bosom' (not 'breast') derived from /&b(b/V)/ 'be dense, thick'.

139. Unknown to me; abstracted from /ġqw/ 'to sink (a well)'?

140. Based on /ġlV/ 'ascend', cf. 137.

141. /ġc/ means 'tree, wood, timber' collectively; 'branch, twig' is a rare secondary var.

142. /ya/ hardly original, cf. on 134 above.

144. Basically monoradical, /m-/.

146. Unknown to me, unless Eg /yn/ 'he said' be meant (a petrified formula in which /y/ may be secondary accretion).

150. /-iy/ probably derived from gn case vowel through prolongation (and -y- as a glide before newly attached case vowel).

151. Used mostly in abbreviated proper names in late historical times.

152. Originally probably collective; regular pl. formation hardly pre-Sem.

154. *Kulturwort*.

155. Unknown to me.

157. ~~Dittoxxunixxx~~ Could be valid, although attestation patchy.

158. /knfr/ (Eth Cush only) probably nasalized var. of /kpr/.

159. Cf. 11 above.

161. Could be valid, although the relevant root is /(h)lk/; cf. also Finnish /kulke-/ 'walk, travel'.

162. Probably derived from the root /kl(l)/ 'be complete(d)'.

163. Unknown to me.

166. Ditto.

172. Weakly attested and possibly onomatopoeic.

173. Based on the root /kr(r)/ '(be/go) round' and spread as a *Kulturwort*.

177. Weakly attested.

178. Unknown to me.

179. Ditto.

180. A wandering word.

190. Cf. 11 above.

192. *Kulturwort*; var. /kad/ (etc.).

193. Cf. 11 above; also weakly attested.

195. Unknown to me.

196. Could be onomatopoeic.

197. Based on the root /kr(r)/ (cf. 173 above).

199. Cf. 11 above.

201. Weakly attested and semantically defective.

202. Unknown to me; but cf. /gl(l)/, 'kr(r)/.

204. Unknown to me.

205. The root is /qṭn/.

208. Phonetically and also semantically possible, although the root is /qlV/; but attested in Sem only.

210. Unknown to me.

211. Seems based on Gn 4:1 where, however, the root is used to create a pun.

212. Seems abstracted from trirad. roots; /q/ and /k/ are not interchangeable in genuine Sem roots either.

214. Could be valid.

215. Unknown to me; normally, /qr(r)/ refers to cold.

216. Known to me from some Eth (Gur and Cush, partly as loan) languages only; normally refers to even or low-lying ground.

217. /-m/ seems original part of the root; attested in Sem only.

218. Attested in Arab only (apart from a remotely related noun in Hbr).

219. Unknown to me, but could be derived from /qšV/ 'to be hard'.

222. /kap/ only known to me; derived from /kp(p)/ 'to bend'; cf. 212 above.

224. Phonetically possible, but geographically remote.

229. Ditto.

230. Looks valid; cf. 215 above.

231. Cf. 11 above.

232. Unknown to me; normally, interrog. pronoun based on /m-/ or /'y/.$_x$

233. Basic meaning of /qwV/ 'to expect, persevere (in efforts)'.

238. Perhaps wandering word.

239. Based on /qwl/ (var. /qhl/) 'to call, speak'; ultimately onomatopoeic.

241. *Kulturwort*.

242. Seems attested in Cush$_x$ only, maybe even there ultimately derived from /qmx/ spread as *Kulturwort*.

244. Weakly attested; normally adjectives are of late origin.

245. Unknown to me, unless the deictic /k-/ referring to what is nearby be meant.

247. Cf. 11 above.

250. Unknown to me.

252. Known to me from Akk only.

254. Known to me in Eth only, as a secondary var. of more original /lxm/.

255. Could be valid, cf. also /rgl/ (var. /'gr/) 'leg, foot', Finnish /yalka/ =; and 161 above.

256. Unknown to me.

257. Apparently prep. /l-/ meant; this may derive ultimately from a root /'lV/, cf. the closely synonymous /'il(a^y)/.

258. To my knowledge, attested in Arab SAr only; cf. 244 above.

262. Weakly attested even if abstracted from lengthier roots (/lpt/ etc.).

267. Unknown to me; the common Sem-Ham root is /m-/; cf. /rwV/ 'to water'?

268. Unknown to me; but the root seems abstracted from Arab /lf'/, /lft/.

269. Known to me from Arab only.

272. Unknown to me, unless indeed /lixlaxunu/ (Bodleian Ms. Heb. d 55 fol. vR 4, cf. my Materials vol. I p. 1) be interpreted as its R-stem; may be transpositional var. of /xlV/, cf. the closely synonymous Arab /nxl/.

273. Cf. 11 above.

275. Cf. 244 above.

277. Could be valid.

278. /ml'/ would be closer semantically (but attested in Sem only; cf. 244 again).

279. /mn&/ attested in WSem only.

280. Unknown to me.

281. **Basic meaning of /mn(V)/ 'to divide, apportion'.**

284. Identical with the indefinite/interrogative pronoun /m-/.

287. Conceivably valid; extended root forms /'mn/, /ymn/.

289a. /n/ appears more original.

290. Basically = 284; negative and prohibitive usage originated from the rhetorical one.

291. Unknown to me.

292. To my knowledge, occurs only as var. to /mr(')/ or /bn/.

293. Abstracted from trirad. roots; but /m(w)t/ could be related, as /r/ is attested as a var. of /t/.

294. Unknown to me; if derived from /mr(r)/ 'pass by/along, flow', original meaning hardly relevant.

296. Abstracted from lengthier roots; spread partly as *Kulturwörter*.

298. /m-/ the only firm element in the 'AA.' entry.

300. Secondary modification of no. 284 above.

301. Phonetics doubtful; even if correct, not widespread.

302. Appears abstracted from trirad. roots of limited occurrence and doubtful relevance.

304. **Known to me from Arab only, perhaps secondarily differentiated from /mc(c/V)/ 'suck, squeeze out'.**

306. /md(d)/ means 'to stretch, extend' rather than 'end'.

309. **Rather limited attestation.**

310. **Ditto.**

311. Unknown to me.

313. /mšV/ refers primarily to preceding evening.

316. **Unknown to me.**

318. **Onomatopoeic.**

320. **Secondary root (cf. /'wr/).**

323. /n-/ may be a secondary root augment, cf. (Hbr etc.) /yc'/.

326. Root var. of /nwr/, from more original /'wr/ (cf.x 320 above).

327. The primary meaning is unsteady movement; quickness is better represented in the cognate /nd(d)/, but this is attested in Sem only.

332. The nasal is a deictic element rather than pronoun proper; but as dem. pronouns are of deictic origin, the comparison may still be valid.

333. /-ān/ is originally collective.

334. Again, /n-/ is a secondary root augment and the original root /g&/, cf. /gw&/, /yg&/.

362. Unknown to me, but may be related to /prs/ 'to split; cloven hoof'.

364. If based on /prV/ 'to slit, cut open', hardly relevant.

338. Wandering word.

367. Unknown to me.

368. Ditto; /plg/ means 'to split^x, divide'; reference to permanent settlement must anyway be late.

339. Cf. 11 and also 364 above.

372. May be valid.

374. Primary meaning of /brk/ is fertilization.

375. Primary root form is /bqr/ and meaning 'to split' (referring to hooves).

376. /q/ and /x/ are not interchangeable nor secondary augments (in prehistoric times).

377. Ditto; and 'door' is a *Kulturwort*.

344. Semantically rather remote.

346. Based on /r'V/ 'to see'.

347. /clx/ ~~an̲x̲e̲x̲e̲n̲x̲x̲s̲i̲x̲x̲~~ would be semantically more apposite; but it is not attested outside WSem.

348. /šhr/ refers primarily to the moon.

349. Cf. 11 above.

350. Unknown to me; /w/ likely to be secondary augment anyway.

353. The primary reference is to the large numbers (of offspring, flocks etc.), not to pregnancy or descendants as such, let alone other relatives.

Editor's Note:
   More responses to this critique will appear in MT-9, in all probability.

Werner Vycichl, Vitalij Shevoroshkin, Stephen Lieberman, Saul Levin, Grover
Hudson, Carleton Hodge, Gene Gragg, Gideon Goldenberg, Alice Faber, Aaron
Dolgopolsky, Abraham Demos, Allan Bomhard, Lionel Bender, Yoel Arbeitman.

July 25, 1989
69 High Street
Rockport, Mass. 01966

No, this is not the beginning of another Circular 1 ! By an emerging
agreement among some of us, including a mail poll of the Board of Directors,
we are starting an experiment in so-called *CA Treatment, only a shortened
and much more rough and ready version of it. An article or review-in-detail
is sent in by someone and it looks promising as a vehicle for discussion and
(hopefully) some shedding of light on a particular topic. Some people may
want to comment very often on things written for MOTHER TONGUE, while others
may hardly cooperate with such an endeavour. We shall see. However, I should
point out to all of you that (a) since you are paying for MT, you will get
more bang for your buck if you cooperate, and (b) since it is YOUR OWN
SPECIALITY that is involved, you will want your opinion registered on the
subject. None of the Muscovites have been solicited on this because it takes
so very long for the mails to go and come. But they will be in the future.

        Will you please read the article and respond in some way in time for
it to reach me by August 15th? I am holding back MT8 just for this purpose.
Ideally you will make a point by point critique of the author's points. In
more general terms we will also publish such comments as "All in all from a
Semitics standpoint the article is correct/half-n-half/poor/ very bad." At
the end of this *CA Treatment which we will start calling *MT Treatment if
it catches on (and *CA is not copy-righted) we hope to have enough good
expert testimonies to be able to say things like "I-S survived his first
test among western Semiticists." or "I-S failed to be convincing in the
Afrasian parts of his etymologies."

        Please respond by computer print-out, typewriter, or pen & hand.
Just try to make sure that your contribution will be legible after
zeroxing.

        If you simply cannot get your contribution back to me before August
15th, but you still want very much to register your opinion, please send it
to Allan Bomhard (86 Waltham Street, Boston, Mass. 02118) and he will try to
publish it in MT8 in October.

        Part of the background to this, and partly why it is so important,
is that Illich-Svytich's work has been central to most discussions of
Nostratic and absolutely crucial to the Soviet claims of exact methods,
precise sound correspondences, and reliable reconstructions. It is no secret
to me -- because of the mail I receive -- that some Western scholars are
unhappy with I-S's etymologies and/or his reconstructions. Now is a good
time to take a hard look at the PRELIMINARY set of etymologies which Mark
Kaiser very kindly sent us in MT5. If you do not have a copy of MT5, you
cannot participate in this endeavour in detail. We all must remember that
this is I-S's first set, not necessarily what an I-S inspired Muscovite
would produce today. Remember also that I-S did not have Igor Diakonoff's
reconstructed proto-Afrasian on hand.

"Precise reconstructions and fancy phonological derivations which are based
on bad etymologies are only sound and fury, signifying nothing." (HF 1989)

REMARKS on A.MURTONEN's COMMENTS ON NOSTR. RECONSTR. of I-S (transl:M.Kaiser)

Some 98 o/o of M[urtonen]'s "comments" are irrelevant: they indicate M's lack of knowledge in certain fields of linguistics and have nothing to do with I-S's work. By the way, I consider quite inappropriate an attempt to review Semitic material without even seeing it: the translated entry heads contain AfAs, and not Semitic, roots. To get to Semitic roots and supporting material of attested Sem. languages, M should take I-S dictionary (at least, 1st and 3rd issues are present in any major library). M considers it legitimate to judge I-S's phonetic comparisons having no knowledge about phonetic correspondences revealed by I-S (Kaiser's translation is not accompanied by phonetic tables present in I-S's 1st volume; if M would know them he wouldn't call,say, the correspondence Alt.g : AfAs ʕ "irregular" (see no.4); - as it is correctly stated in the table on I-S p. 152, this correspondence is quite regular when in clusters with sonorants).

M's lack of knowledge of non-Semitic AfAs languages clearly shows on those many occasions when he sais "unknown to me" about AfAs roots reconstructed by I-S: in vast majority of such cases the AfAs roots are reconstructed on the material of Chadic, Cushitic and/or other non-Semitic languages of the AfAs family. Still, in some cases some Semitic l-ges are used, but corresponding words are "unknown" to M (nos 40, 81, 98, 139, 155 and others).

M's profound lack of knowledge in diachronic semantics shows on many occasions; I cite a few: 3. M sais that the meaning 'to look' might be "abstracted from secondary meanings" such as 'to stay' or 'to split'. - 8. The meaning 'grab, catch' - from 'pure, clear'. - 31. 'Knee' - from 'fertilization'. - 33. 'Split, cut, point' - from 'to ascend'. 61. 'Nearby'-from 'be minute,fine (ground/flattened)' etc.- 63. 'Cover, close, press' - from 'silence' or 'faint noises'. - 67. 'fish' - from 'production of abundant offspring or crop'. - 84.'Bold' - from 'to shave'. - 138.'Lap, bosom' - from 'dense, thick'. -139 'Water' - from 'to sink (a well)'. - 140 'Burn offerings, flame' - from 'ascend'.- 162.'Daughter (sister)-in-law, bride' - from 'be complete(d)'. - 173 'Lamb, sheep' - from '(be/go) round'. - 239. 'Tribe' - from 'to call, speak'. ████████████ ██████████████████████████████ 284 and 290: Derivational nominal formant = = prohibitive and negative particle. - 368. 'Settlement, dwelling' < 'split, divide'.-374. 'Ask, pray, bless' < 'fertilization'.375. 'Cattle, bull' < 'split'.- 348. 'Be awake' < 'moon'.--- It is clear, of course, why M commits all these blunders: he bases on phonetic shapes of (Semitic) roots, not on their meanings. Phonetically similar or identical roots should be, according to M, genetically related. This is not XXth-century linguistics, not even XIXth-century. --- Of course, I-S has based his reconstructions on very exact phonetic and semantic correspondences - some of them known before him, some revealed by him.

M shows lack of knowledge of such important works on AfAs (including Semitic!) comparative linguistics as Compar.-Hist. Dict. of AfAs Languages edited by I.M. Diakonoff (1st issue: 1981; 2nd: 1982; 3rd: 1986), papers by Diakonoff, Militarёv, Stolbova, Porkhomovsky, Orёl, Dolgopolsky et al.; and I mean not only papers published in Russian, but also in English (published several years ago). Hence even a few valid comments made by M contain nothing new: corresponding observations have been already made. Take no 6: AfAs Dict.(issue 1, no 118) states that the meaning 'blind' of AfAs *bll- is a secondary euphemism. Or no 141: As Dolg. has shown, AA/Sem.*ʕiĉ- 'tree' should be removed from this set and replaced by Sem. *xass- 'leafed branch'. M has no knowledge of independent reconstruction of AfAs lateral obstruents (ŝ,ĉ,ç̂) by the Soviet team and by Dolg.; this makes irrelevant any remarks concerning Nostr. roots with sibilants and affricates if these remarks don't take in consideration the newly reconstructed AfAs lateral obstruents - alongside other affricates and sibilants. M lacks knowledge of the reconstruction of AfAs vowels (primarily based on the reconstructions of vowel systems of southern AfAs languages) by the Muscovite scholars. This reconstruction is a very strong corroboration of correctness of I-S's Nostratic reconstructions made some 25 years ago.— Cf. important recent paper by Orёl (VoprJaz. 5, 1988) on the identity of AfAs and East Nostr. (Ural. etc) vocalism. All this makes meaningless many comments by M; cf. no 8: what M sais about 8 is all wrong. AfAs had not only b and r in this root but also a (see AfAs Dict. issue 1, no 93), and the meaning is 'grab, grasp' etc.: all confirms I-S's reconstruction of Nostr. *bari 'take' (as in Alt. *bari- 'take into hands', etc.).

M applies a double standard to his own conclusions when compared with I-S's.
M allows humself to derive the meaning 'deaf' from 'circular movement' (no 74)
and other such monstrosities (see above) but he does not allow I-S (no 28) to
compare AfAs root with the meaning 'be excessive' to Kartv. cognate with the mea
ning 'be sufficient' (both roots are quite comparable, in a fact). He wouldn't
allow to combine 'bitter' and 'sour' in one root (54) despite wide-spread evi-
dence to the contrary (cf. even in Arabic: O.Stolbova [Comp.-Hist. Phonetics and
a Dict. of WChad. L-ges in: Afrik. istorič. jazykozn., M 1987, p.2o3, no 54o. -
WChad. *HV-čamV 'bitter' is related to 54 [which has to be re-reconstructed as a
root with initial *č]; WChad. l-ges show presence of 'bitter, sour, astringent'
in one word . Cf. typology,e.g.,Sal.:Squamish l∂s = sáiam 'bitter,sour'. -
On occasions, M derives nouns from adjectives (cf. 219: 'bome'< 'hard' [apparent
ly, the other way around]) but he would'nt allow I-S to reconstruct a root with
the meaning 'high' (9) since "adjectival meanings are secondary"; cf. also his
remark about 244 ('short'). In reality, there are adjectives ('dead';'short/
/small'; 'wide/broad'; 'high' and some others) which belong to the most stable,
most archaic, basic lexics. I-S's reconstructions are very good - now supported
by additional Nostr. data, as well as by external comparisons with non-Ns l-ges.
    On very many occasions M formulates captious objections, - showing, in fact,
his own lack of understanding of the ways of linguistic evolution. He does not
see that words for body parts are exceedingly stable and archaic; so he objects
to I-S's reconstruction of AfAs words for 'knee' (see above), or 'foot' (etc.;
he derives this root from 'bend': 222); same about such basic words as 'deaf',
'look', 'know', 'nape of neck','greasy/smear', 'wash', 'end', etc. (In all these
cases I-S demonstrates solid evidence of Sem. and/or non-Sem. AfAs languages, to
say nothing of non AfAs Nostr. languages. New data confirm I-S's reconstructions
cf. also I-S 24 - and AfAs Dict., issue 1, no 117; I-S 50 - confirmed in a re-
cent paper by Orël and Stolbova on Cush., Chad. and Eg.; I-S 63 - and AfAs Dict.
no 154 [the exact meaning: 'smear, cover with smth. sticky']; I-S 73 - and AfAs
Dict. 2, no 143,[the root was not just *dm but *dum- = Kartv. *dum- 'be silent']
I-S 76 - and AfAs Dict. 2, no 176; I-S 136 - and Dolg.'s reconstr. AfAs *?it-
'eat'; I-S 146 - and Orël/Stolbova's data [the root is *jan- 'tell', not just
*jn, - which is a welcome confirmation of I-S's Nostr. reconstr.: now *janV],etc.
Where M sais that the AfAs root shows just one consonant,it usually shows two
(cf. 129, 144, 267, 75 and others; moreover, recent reconstructions of AfAs vow-
els give further confirmation to I-S's etymologies; cf. above).
    M is certainly wrong when he objects to "onomatopoeic words". Originally "des-
criptive" words (and there are more such words than we usually think) became nor-
mal (and stable) words in ancient languages: they are not descriptive
at all in languages we use for reconstructions. They follow phonetic rules in
the way all other words follow.I-S did not include in his dict. regular onom.word
    M is wrong objecting to "Kulturwörter": Roots used by I-S follow regular pho-
netic rules (see above) and thus do not behave as borrowings; they are no Kultur-
wörter (on some occasions these - and other - words show interphyletic genetic
relationships). I-S was very apt to distinguish between borrowings and words in-
herited from a proto-proto-language: see his paper on Semitic borrowings in IE
(in Probl. IE jazykozn., M. 1964). Thanks to him it became possible to separate
cultural borrowings in Proto-IE from inherited words (this also lead him to the
idea [not new in itself] that IE homeland was in Anatolia: not far from Semites).
NB) I-S 192 *kadA (=*k'adV) 'plait' (NB I-S's comments). * * *
    Nothing in M's comments make them interesting and useful; to me, it is one mo-
re attempt to discredit I-S; none of previous attempts succeeded. All this in
spite of the fact that I-S wrote his work a quarter century ago; so it, obvious-
ly, requires some corrections. Such corrections are being made by scholars who
know well both Nostratic and its daughter languages. What is more interesting
is the fact that the recent progress in deep reconstructions brouhgt not only
confirmation to I-S's results but also added elements which were not known at
I-S's time but had been predicted by him. --- What is needed now is an English
translation of the whole Nostr. dictionary. Publishers are ready to publish it,
but they are not paying for translation (still, 67 entries .. have been re-
cently translated by our able student John Masteika). --- As for a "test", I-S
does not need any: he past his test many years ago with flying colors. His work wa
praised by Collinder, Menges, Poppe, Garde, Birnbaum et al.- and that's in the West

Some "detailed remarks" on M follow [Abbreviations Diakonoff's (ed.) Comparative-historical dictionary of Afrasian languages , 1st issue of which appeared in 1981, 2nd - in 1982, 3rd - in 1986, I cite as Dk1, Dk2 and Dk3 accordingly].

Detailed remarks: 3. Dk1 p.62 sq. (no 78) confirms I-S's reconstr. fully (Dk reconstructs the primary root as *bVḳ 'observe';"extended" root : *bḳj/w/?; present in Aram., Arab., Cush., Egypt.; also extentions *bk-č, *bḳ-r/l (former in Ugar.,Hebr.,Phen.;latter in Akk,Hbr,Aram.(Syr.) etc.). I-S had, essentially, similar data. Note that M cites wrong roots (s.above

12. The AfAs root is *bač' (Sem.,Cush.,Berb.): Dk1 pp. 58-9, no 72.

13 and 20. I-S has shown, on several occasions, that a language tends to "combine" two, originally different, roots, provided they are similar both in sound and meaning. Only external comparison can separate such roots.AfAs *bjl (i.e.,*bil-) is present in Eg., Cush., Chad.; the meaning 'cloud' is present in Cush. and Chad. and fits Ural. *pilwe, Alt.*buli- 'cloud'. I-S stresses here (entry 13) that a secondary meaning 'rain, water' seems to have developed "under the influence" of AfAs *b(w)l 'to moisten/damp'(no 20)

16. AfAs (sic) *?bl is present not only in Sem., but also in Cush. and Chad.

20. See 13. - Note that AfAs *b(w)l is present both in Sem. and in Berber.

22. and 23. I-S indicated (last lines of entry 22) that 22 and 23 are related.

24. I-S puts "? AfAs [his "S-H"] br- 'to boil'". The correctness of this reconstruction and comparison with Ural. *pura-, Dravid. *pur- and IE *bhreu- (all roots with -u-) is confirmed by Diakonoff's group: see Dk1 p.95 (no 117) where a root variant with *-u- (sic) is present: AfAs *bwr 'to boil'.

28. Data of diachronic semantics (based on observations of very many different languages) confirm correctness of I-S comparison. On the other hand, Murtonen's unifications of phonetically identical (or similar) roots is absolutely unacceptable (see General remarks).

30. AfAs *bnt' is not present in Sem.; it's present in Eg. and Chad.

31. Dead-wrong! Words for body parts are most archaic and stable! Cf. Dk1 p.87.

32. Sem. has both r and n; the fact that r (and not n) is the primary sound in our root is confirmed by external comparisons (IE *bher- 'child',Krtv *ber-).

33. How can a meaning like "split, cut" originate from "to ascend" ???! - Dolg: Nstr *šäl(V)XU > Sem. *šlX 'cut asunder' (X=x),Krtv *š/sXal-,Ur (FU) *šäl[ü]

36. I-S sais that the comparison is possible only if Sem. -t is a suffix.- Sem. *s is reconstructed not only by I-S but also by Diakonoff et al. cf.,e.g., table in Afrik. istor. jazykozn.,M. 1987, p. 24/25. - Dolgopolsky confirms I-S's *st-r (-r = suff.) but considers it as a variant of Sem. *št-r < AfAs (as shown by SCush. *ša(a)ṭ [ṭ = retrofl.] 'cover up', Eg. št? 'to hide'): see Proceedings. 4th Internat. Ham-Sem. Congress, Benjamins, 1987, p. 203.

37. I-S's reconstr. seems correct (though Berber root is different).

38. Chadic data (unknown to M) confirm I-S's reconstr.and comparison with Krtv.

40. Berber data confirm semantic reconstr. ('herd of wild [sic] animals'). I-S cites Sem.: colloquial Arab. ṣi/uwâr 'herd of wild oxen and cows'.

42. Dolg.: Sem. *š?n 'know',*šnj 'see' (Krt *čan-,Ür.*š/šunV)[≠Eg. swn,Hs. sáni]

44. Not only in Sem, but also in Chadic! In any case, I-S puts a "?".

50. The root is confirmed in recent papers by Orël and Stolbova on Cush.,Chad. and Eg.- Where Arab. t,Aram.t,Hebr. š,etc,are present,Diakonoff gives *č.

51. See 50.- Modern research allows to reconstruct two Nostr. roots on I-S data

59. The primacy of locative meaning is confirmed by Cushitic (Bilin,Saho -d,etc

60. I-S discusses Berber, Cushitic and Chadic. (1c)

61. I-S does not discuss Semitic words here ! (Only Cush. and Chad.; very archa

62. I-S does not say that AfAs *dlx refers to waves or sea (Sem.= 'agitate/rippl

63. Primacy of Sem. *dm- is confirmed by both Berb. and Cush. Both Semitic and IE show an archaic meaning "(cover with) dung" (Engl. dung is related).I won der how 'cover, close, press' can be "abstracted" (=M) from a "basic meaning" 'silence of faint noises, with subdued activity'. -See also 73 (a different root)

67. I wonder how Sem. *d(j)g (i.e., *dig-) 'fish' [= 'fish' in IE and Alt.] can be "derived" from the root "production of abundant offspring or crop"(M)?

73. M identifies Sem. *dm- 'cover' with *dm- (<*dwm !) 'be quiet, silent' (63) just because both roots are phonetically identical !! (but even this is not true). - AfAs (= Sem. and Cush.) *dum- = Kartv.*dum- id.(= Alt. *düŋä-)!!!

Dk2#/5/ *dVm- 'smear, cover with' smth. sticky

Dk1=/43/ *dil/ɛm

74. How can a meaning 'deaf' be derived from 'circular movement' ??! - AfAs *dwr
    [dur-] and secondary *dr- is present in Berb.,Cush.,Chad. (AfAs *dur- = Krtv.
    *dur- 'deaf'). The meaning 'deaf' is present in all three above branches.
75. Recent research confirms I-S's reconstruction. It seems, AfAs root was, actu-
    ally, *wdꟲ/*wdj (< *djꟲ^W ?), cf.IE *dheiH^W (H^W being a <u>real</u> labiovelar, refle
    cted either as "length" or [w]), Krtv *dw-, all meaning 'put' (<Nstr *diꟲu).
    The AfAs root was present in Sem. (*wdꟲ/*dꟲ),Eg. (wdj) and Chad.(*d[u]H-).
76. Wrong; the root is also present in Cush. (Oromo du(?) 'die'),Berb.,Chad.! In
    Dk2 no 176 it is reconstructed as AfAs *dVhr (because of ? in some daughter
    languages; note independent reconstr. of Dolg.: Nostr. root with *-H-;*H in IE)
    [It is clear that Sem. *dwj/*dw? = IE *dhweiH-; the root should be **diꟌw?V]
81. Quite wrong. AfAs root is present in Sem. and (mostly) in Cush.; the meanings
    in different l-ges are: 'back of the head', 'back (adj.)', 'anus','(be)hind'.
82. The primary meaning in Sem. is connected with intense light (not heat); but
    cf. <u>other</u> AfAs l-ges: CentrCush. ('day'), Chad. ('sky', 'day'), etc.- Cf. 95.
84. The <u>primary</u> meaning in Sem. is <u>not</u> 'shave' but 'be bold','bold (spot)'. The
    semantic change from (Nstr) 'smooth/shiny' to 'bold' is totally normal.
87. AfAs *gwP is based on Sem. *gwp (sic) and related roots in ECush. and Chad.
88. The AfAs root *g(w)ꟲr is "unknown" to M because it is not present in Semitic.
    But it is present in <u>other</u> AfAs l-ges: Cush. (Saho gŭr etc),Chad.,Eg.(/Krtv*gʒr)
90. The root is "unknown" to M because it is present in Cush and Chad.- As for
    Sem. /gŭr/ mentioned by M, it might belong to Nstr *gUjRä (93) 'wild animal'.
95. Roots 82 (Nstr *gE/hr/a 'dawn') and 95 (*gUrV 'living coals') are, most cer-
    tainly, different: 82 has no -w- in AfAs, and the meaning is <u>not</u> connected
    with heat (cf. also IE *g̑herH- 'dawn', Alt. *gĒrä id.), whereas 95 <u>has</u> -w- in
    AfAs (Chad. *gwr; similar in Cush.), and the meanings of related words are
    'burn','roast','catch fire' (Cush.), 'set fire!,'coals','ashes' etc. (Chad.);
    cf. IE *g^{W}her- 'living coals','burn,hot';Alt *gurV- 'liv. coals;catch fire'.
    Hence in AfAs: *ghr 'daylight, day' (82), but *g(w)r 'fire, coals' (95).
98. Sem. *Ꟍwr is, most certainly, 'deep water' (cf.,in attested Sem. languages:
    'depth','submerge','be absorbed' in Arab.;'subsoil water' in Ug.;'lake':SArab)
100. Not so. The initial h- (<*h) is present in Arab. (hwj),Ug.,even in Old Hebrew
     hawā́ 'evel desire'; also in Cush.(Somali hawo 'desire,passion' etc) and Eg.
101. According to Dolg., two Nstr roots are "mixed" here : one in *h-, one in *x-.
111. ̶T̶h̶e̶ ̶o̶r̶i̶g̶i̶n̶ ̶ The original "past" meaning is present in EChad.,Cush.,Berber.
121 and 122. I-S stressed the connection, of course. *?- <u>is</u> certainly present:122
123. Sem. has several secondary meanings; original meanings are present in Cush,Br
124. "Day" only in IE. I-S cites AfAs *?m(-?)r 'daylight; see' under a "?".
125. Originally descriptive, the root developed <u>non-descriptive</u> meanings in l-ges.
126. I-S does <u>not</u> reconstruct the meaning 'witchcraft' for AfAs (Sem.);words:Arab.
     ?rb 'be cunning' etc, also in Old Hebrew and Ug.; <u>all</u> with ?- (diff.sources!)
127. Semantically <u>quite</u> close (similar semant.connections in <u>many</u> languages).
128. Both *? and *I are well reconstructable for AfAs (as Orĕl has recently shown
     [VoprJaz. 5, 1988], AfAs <u>vocalism</u> is both well reconstructable and <u>identical</u>
     - in related roots - with Nostr. (as in Uralic, Alt.,Drav.)). Our root is well
     attested both in Sem. <u>and</u> Cush, <u>and</u> Chad.;Present in <u>all 6</u> Nstr languages !
129. Though the Nstr root has only 2 phonemes, they <u>both</u> are reflected in AfAs
     *?j (< Nstr *?e). <u>Exact</u> correspondences (phon.,semant.) in Ural.,Drav.,Altaic.
130. Again, <u>exact</u> correspondeces in AfAs (Eg.,Cush.,Chad.), Alt.,Drav., IE.
132. Sem. 'there is ...' fits quite well; as usually, more archaic meaning in non-
     Sem. l-ges (Berb., Cush., Chad.),- e.g., 'arrive at a place': fits well Uralic
133. "Unknown" to M but present in Eg., Berber, Cush., Chad.: take I-S and see !
134. Again, archaic meaning - in non-Sem. (Berb.,Cush.,WChad.); Sem. <u>fits</u> as well.
135. Apparently, <u>not</u> so: phonetic correspondences are very exact,- fit for cognates
136. Don't mix two roots! Root 136 is, actually, *?it- in AfAs (no w in Cush.)=Dlg
137. Rarely attested words are frequently most archaic. "Mountain" is the regular
     meaning in ECush. (archaic). This "evidently secondary" is just plain wrong.
138. Why should a body-part word be derived from an adjective ? The meaning "w.'s
     breast' is, most certainly, present in AfAs (shown by Berber and Chadic !)
139. "Unknown" again? Strange: it is present in Arab. as well, - though the archa-
     ic meaning 'water' is Cush. (*ꟲk'w = *ꟲqw). How can such stable word as "wa-
     ter" be derived from "to sink (a well)" ?

V. Shevoroshkin

Comments on Murtonen's Comments on Kaiser's translation of Illič-Svityč's Nostratic
Etymologies

Alice Faber

1. Murtonen is making no evaluative summary comments on Illič-Svityč's basis for
including Afroasiatic in Nostratic, but the overall effect of his list of quibbles is to cast
doubt on that inclusion. Bearing in mind that we don't see (at least in the MT version of
Illič-Svityč's lists) the intra-Afroasiatic evidence, I have to concur in this doubt. However,
if it were to turn out that the many AA roots cited by I-S which I am unfamiliar with are
solidly reconstructible on the basis of **extensive** attestations in Cushitic, Omotic, Berber,
Egyptian, and Chadic, I would, of course, stand corrected.

2. In general, I find it disturbing that Nostraticists (?) appear to assume a genetic relationship
between AA and the other language groups rather than pose the broader question in
terms of which language stock AA is related to at the next level (next higher node). Being
unwilling to accept the prospect of multiple origins of human language, I take it as given
that there is some language stock to which AA is more closely related than to all others.
But, I'm not at all sure that the comparisons adduced by I-S and other Nostraticists are
inherently any more convincing than a comparable list of potential points of comparison
with Nilo-Saharan or Na-Dene would be. I am also bothered by the practice of using
biconsonantal AA roots for extra-AA comparisons when these biconsonantal roots have,
apparently arbitrarily, been extracted from **reconstructible** triconsonantal roots (except in
cases where the remaining consonants itself has a clear meaning [e.g., s̠- or -s̠ representing
a frozen causative]).

3. Given that my work on Semitic and Afroasiatic comparisons has focussed on items
containing voiceless sibilants (*Glossa* 15: 233–262, 1981; *Journal of Semitic Studies* 29: 189–
224, 1984; *Diachronica* 3: 163–184, 1986) and on negative and interrogative words (32nd.
ICANAS, Hamburg, 1986), my comments on I-S, and on M's remarks will, for the most
part, be restricted to these items. In the Proto-Semitic forms I cite, I will follow the
reconstruction in my JSS article as well as in my forthcoming book (Eisenbrauns, 1990?
1991?), and use *s (rather than *š) for the ancestor of Hebrew š̠, *ɬ for the ancestor of
Hebrew ś̠, and *c (rather than *s) for the ancestor of Hebrew s̠. This is, as well, the
reconstruction advocated by Diakonoff (*Afrasian Languages*, Nauka, 1988). I have
converted all of the forms cited by M to this system. (Many forms I cite are taken from
various works by M. L. Bender, C. Ehret, J. H. Greenberg, P. Newman & H.-J. Sasse, and
others, to whom I apologize for the omission of explicit references in an informal
communication of this sort.)

#32 'child'. As M notes, the Semitic forms with /r/ rather than /n/ are secondary (D. Testen,
JNES 44: 143–146, 1985). It should be noted, though, that forms reflecting br rather than bn
are also attested in Cushitic. Greenberg's (1963) common Nilo-Saharan list includes
bar/mar 'boy' (#24).

#33 'split, cut'. While several roots reflecting *sl- referring to cutting or to pointy objects are
found in Semitic (e.g., *slḥ 'sword', *slṭ 'lance'), they appear to be WS in origin, **at the**

earliest, and not AA. Forms containing *cl- (e.g., *clʕ 'stone') would, if they were **reconstructible beyond Central and South Semitic**, provide more appropriate comparisons, were it not for the fact that, as M notes, Semitic *c represents an innovated phoneme, and not one inherited from AA.

#36 'hide'. I have no opinion regarding M's suggestion that Semitic *ctr is a secondary formation from *cwr. Regardless, *ctr is West Semitic, at the earliest, and hence should not be compared with other Afroasiatic forms, let alone at further remove. In any case, as noted above *c is not an AA phoneme.

#50 'pus, slush'. Like M, I am not familiar with any forms of *θr(y) meaning anything other than 'moist, damp'. However, I disagree with M's contention that *θ is not PS; about 20% of the **reconstructed** lexical items with *θ in my corpus have relatively solid potential cognates in other Afroasiatic language groups. I will admit, however, some uncertainty regarding the nature of the ancestor phoneme.

#51 'smell, odor'. While *θyn is solidly reconstructible for PS, and perhaps, on the basis of Cushitic cognates, attributable to PAA, the forms **unambiguously** mean 'urine, urinate'. Hence the comparison with Uralic is far-fetched on semantic grounds. G. lists N-S *sad* 'urine' (#143).

#56 'look after, guard'. I know of no forms reflecting *θr- with an appropriate meaning. There is, almost certainly, no such PS form.

#121 deictic particle. I agree with M that some forms with ʔa are secondary to ha, itself secondary to sa.

#127 'fire'. I don't find M's suggestion that *ʔs(t) is onomatopoetic in origin plausible. Given the clear PS forms, and Bender's attribution of *at 'burn' to Proto-Omotic, the item is conceivably PAA. But, given the tenuousness of the Altaic form given, based as it is on a single language, cultural transmission between Semitic and IE cannot be discounted.

#128 negative. The Semitic negative *lā has no AA cognates (with the exception of Agaw forms which are to be treated as loans), and is probably to be connected with the asseverative *la (on which, see Huehnergard, JAOS 103: 569–593, 1983). Likewise, the prohibitive *ʔal is no older than WS. Hence, neither can be compared here (*pace* M).

#129 negative. The most appropriate PS reconstruction for this item is *ʔay. Elsewhere in AA, this negative is, to my knowledge, restricted to Burji (Cushitic); thus, it is not likely to represent an original negative.

#132 'be at a place'. As M notes, verbal forms of this root in Semitic are secondary. However, this is not, in and of itself, a barrier to longer-range comparison.

#142. WH. Semitic, Cushitic, Berber, and Omotic unambiguously reflect *ʔay. I know of no forms reflecting *ya. Thus, AA forms should not be, as M notes, compared with the other forms given by I-S.

#196. 'cut'. There is no PS form reflecting *ḳc meaning 'cut'. In any case, as already noted, PS *c was not inherited from AA.

#219 'bone'. Rather than deriving 'bone' from 'hard', as M **very** tentatively suggests, the reverse is more likely. 'bone' is attested in Egyptian, Cushitic, and Chadic, while 'hard' is found only in Egyptian, Central Semitic and Harsusi (Modern South Arabian).

#232 'who'. k̲ as an element in AA interrogatives is widespread, but is, to my knowledge, found only in stems meaning 'when, why, where, how?'. I know of no forms reflecting *ḳ.

#257 locative. If Semitic *l and *ʔil(ay) are, as M suggests, are to be related, it seems to me equally likely that the latter is secondary, derived from the former by compounding with the preposition *ʔin(na), attested as an independent form in Akkadian (?) and Eblaite and in compounds in ESA (mn, bn, ln, all meaning 'from') and, perhaps, in Central Semitic. A stronger barrier to the comparison, however, derives from the fact that all of the AA forms with which I am familiar reflect either a dative or a directional meaning, while all of the other candidate Nostratic forms given by I-S appear to be pure locatives.

#273 'tongue'. I know of no forms reflecting *ls within AA meaning 'lick,' other than some Ethiopian Semitic forms resulting from elision of a medial laryngeal, although this gap does not constitute a barrier to wider-range comparison. I see no basis for treating this item as onomatopoetic. G. lists N-S *lit* 'tongue' (#140).

#290 negative/#300 WH. Prohibitive reflexes of AA *ma are found in Egyptian and Cushitic, and more general negative reflexes in Semitic (relics), Cushitic, and Omotic. *ma may also reconstructible as the AA WH element, but I know of no relevant Omotic forms. There is thus no basis for M's assertion that the negative is secondary to the interrogative, although the assertion may, of course, be correct. If anything, though, the negative is older than the interrogative. It should also be noted that *m negatives are found in language families which, I believe, have not been connected with Nostratic (e.g., Nilo-Saharan).

#362 'nail'. AA *pr more likely meant 'wing' or 'fly'. G. lists N-S *fat* 'wing' (#156).

#374 'ask'. AA (most likely just PS) *brk is probably related to the 'knee' family. I don't understand M's relating it to 'fertilization'.

#347 'be favorable'. AA *sl- presumably refers to PS *slm 'healthy' and WS *slm 'peace' and *slw 'be calm'. WS forms reflecting *clx 'forgive', alluded to by M, may be comparable with Akkadian forms meaning 'sprinkle', borrowed, according to Lieberman (1974, #648) from Sumerian.

#348 'be awake'. AA forms reflecting *shr meaning 'be awake' are of extremely limited distribution in Central Semitic (Arabic, Modern South Arabian, and Jewish [i.e., late] Aramaic). A more appropriate AA comparison might be PS *sḥr 'dawn', although WS *ṯhr 'new moon'/PCushitic *ḷeeh 'moon' is also possible.

Saul Levin

Remarks on the Recent Circulars from Mark Kaiser and A. Murtonen

Kaiser and Murtonen have both done a worthy service to linguistics, one by presenting more than 350 Nostratic etymologies of Illich-Svitych, with the Russian glosses translated into English, and the other by evaluating virtually all of the etymologies that involve Afro-Asiatic. Their treatment of the individual etymologies is brief but surely useful.

Murtonen's frequent comment *"Kulturwort"* calls for some clarification. Lurking behind it, I suspect, is the notion shared implicitly, if not explicitly, by many linguists that the vocabulary to be identified in a remote proto-language must have consisted of such basic items as no human population could fail to express by some word or other -- e.g. external parts of the body and some simple motions. Such words would be least liable to get dislodged from the vocabulary by subsequent bilingual contacts, while on the other hand any word serving to express some higher development of culture would easily diffuse through trade or the immigration of individuals -- i.e. through bilingual contact. This model is plausible, except insofar as it leads to the absurd conclusion that a proto-language community had no culture, nothing to differentiate it from the rest of mankind. That becomes a self-defeating hypothesis, weakening our motive to do linguistic research, since our curiosity to trace languages as far back as possible springs in large part from a hope that we shall thereby discover things about the life of our remote predecessors at various stages, things that we either share with them or do not share -- including the intangibles that are inaccessible to archaeology. Take that away from linguistics, and you are left with the bare bones (so to speak) of languages. Without involvement in a culture, material and immaterial, they are much less interesting.

My most serious reservation, however, concerns what both Kaiser and Murtonen have symbolized, negatively, by omitting asterisks. Any reader who stops to think will, presumably, take *everything* cited by Kaiser or Murtonen to be a reconstruction, not a form on record in any actually known language. So, for economy, they have excusably dispensed with thousands of asterisks. But is it quite true? Are the Kart[velian] words (whether whole or partial) mere reconstructions, or are they quotable from texts (transliterated from the Kartvelian or Georgian alphabet) and from oral recordings? Being ignorant of the languages of the Caucasus, I am puzzled as to this important point, but am led on to a more general reflection: We must not confuse the data with any and all interpretations built upon the data. Every reconstruction has standing only as an interpretation.[1]

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[1]See my article, "The Romance and Indo-European Models of Comparative Linguistics," *The Thirteenth LACUS Forum 1986,* pp. 549-557.

What is conspicuously lacking, and would need much more space than Kaiser's twenty-three pages and Murtonen's seven, is the data -- i.e. the words known to exist in certain documented languages -- on which the reconstructed Indo-European, Afro-Asiatic, Altaic, Dravidian, and other proto-language forms are based. Those data are indispensable if each of us is to arrive at an informed opinion about the trustworthiness of the reconstructions that Illich-Svitych used for his subsequent reconstruction of Nostratic. Some researchers -- e.g. Indo-Europeanists -- might be willing to accept the Indo-European reconstructions as well established, but not the Afro-Asiatic, either because they themselves are ignorant of non-Indo-European languages or because they know enough about Afro-Asiatic studies to be suspicious of all or most Afro-Asiatic reconstructions. Anyhow, **reconstructed forms have no validity except by reference to actual forms**; so any discussion is simply up in the air if it is limited to reconstructed forms and their attached meanings, which -- for all that we are told -- may also be merely reconstructed.

Well informed opinions, furthermore, will often disagree methodically about how a particular reconstruction should be formulated. What Illich-Svitych gives as "IE. gᵘeihᵘ-/gᵘiehᵘ- 'be healthy, live'" (#168), is *kʷīu̯- according to Gamkrelizde and Ivanov.[2] It might be argued that for the secondary reconstruction of the remote Nostratic either formulation is usable, or whichever happens to be more compatible with the non-Indo-European languages. Here Illich-Svitych gives "Ural. kōja 'fat, fatty'".

A danger which my experience warns me of in the method of Illich-Svitych and other Nostraticists (including my friend Allan Bomhard) is that by relying mainly on what they judge to be Indo-European proto-forms, Afro-Asiatic proto-forms, etc., they overlook the most revealing correspondences which show up between particular languages, very distantly related on the whole. E.g. the Old English feminine noun [ʔ]*eorðan* 'earth' (accusative/genitive/dative)[3] most closely matches the Arabic feminine أرضًا (ʔarḍan) (accusative); but its diphthong *eo* (surely accented, though the accent was never written) recalls rather the alternating (ɛ/ɔ) in Hebrew אֶרֶץ (ʔérɛc), אָרֶץ (ʔɔ́rɛc).

I think it is premature to speculate whether such fine, precise

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[2]Индоевропейский язык и индоевропейцы (Tbilisi, 1984), II, 465.

[3]The initial consonant, though unwritten, was formerly pronounced as it still is in German [ʔ]*Erde*. The alliterative versification of Old English, like other early Germanic languages, implies that in such verses as *se ælmihtiga eorðan worh*[te] 'the Almighty wrought earth' *(Beowulf* 92) a consonant sound began the two alliterating words that are written with different initial vowels; see "The Glottal Stop in the Germanic Languages and Its Indo-European Source," *General Linguistics* 24 (1984), 233-235.

resemblances can be traced back to a proto-Nostratic source. But in any case they are too substantial to be sacrificed or shunted aside until someone figures out an Indo-European proto-form for [?] *eorðan* and an Afro-Asiatic proto-form for the Arabic and Hebrew words. For these words, and quite a few others documented in a number of Indo-European and Semitic languages, have more to them than even the best of Illich-Svitych's reconstructions.

One of the best of his is #5, to which I would add the closest actual correspondence, Aramaic בְּרַק (bəraq) : Sanskrit भ्रा ट् (bʰrāṭ) 'he flashed, made lightning; it gleamed' (present भ्रा ज ते (bʰrájatē) 'he/it gleams, flashes'). The attestation of this Aramaic verb-form, however, is somewhat uncertain -- perhaps only in later Syriac -- while this tense of the Sanskrit verb is rare altogether but attested in अ भ्रा ट् (ábʰrāṭ) with the prefix (á-) expressing past time and regularly optional in early Sanskrit poetry. The fullest morphological correspondence, although semantically vague or detached, is

Latin *fulgus* 'flash of lightning' : Hebrew בָּרְקַת (bɔrqát) 'emerald'
(a rare, probably archaic
equivalent of *fulgur)*

Saul Levin

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(August 10, 1989)
Ed.

3

Dear Hal,

Proper reaction to (more appropriate than response to) Murtonen's comments on Illič-Svityč would take a book. I confine my remarks to a few samples of what needs to be said, preceded by some prefatory comments.

Preface. My work on proto Lislakh bases has been going on for some years, and about seventy bases have been presented in material published to date. While many of these have been discussed in IE terms, they are LL bases and are to be found or expected elsewhere in the phylum. Many more are given in articles in press; others are in an advanced state of readiness, while hundred of raw base files await analysis. Any discussion by me of AAs (Lis-ramic) or LL will be done in terms of such bases wherever I have found them to be reconstructable. As the presentation of a single base can be 1 to 40 pages, it is clear that priority (for me) must be given to the publication of them as LL units and not just as part of one's criticism of IS or anyone else. LL as a phylum has been given support by a study I made and presented to the African Linguistic Conference (U of I, 1989). For thirty Omotic sets (most with reconstructions) from Bender we have the following number of cases where cognates can be cited:

Omotic	Egyptian	Semitic	Berber	Chadic	Cushitic	Indo-European
30	26	22	14	29	23	28

Due to the unevenness of the data, one cannot draw statistical conclusions as to the relative closeness of the branches, but the IE showing, by any count, is impressive.

The LL bases must be understood in terms of consonant ablaut, so that a base like **b-l may have forms such as −

LL base	Likely reflexes		Base	Reflexes	Base	Reflexes
**b-l	b-l	IE w-l	**bH-l	b?-l, bh-l	**Nb-l	m(b)-l
**b-lH	b-r,	w-r	**bH-lH	b?-r, bh-r	**Nb-lH	m(b)-r
**b-Nl	b-n,	w-n	**bH-Nl	b?-n, bh-n	**Nb-Nl	m(b)-n

Note the ablaut set l − lH − Nl, usually represented by l − r − n. Note also that all glottalized/ emphatic/ aspirated consonants are secondary, as are many nasals. These nine variants are usually treated as separate roots, which results in confusion. Needless to say, there are a great many questions raised by the bases deduced and the ablauts thereof. One does not yet know of any connection between **k-p 'hand, seize', **k-b 'sole (of foot), foot', **g-b 'side, hand', or others where there is both semantic and phonetic similarity but no known derivational track between such similar but divergent forms. Despite the problems, I consider consonant ablaut to offer the best approach at present and do not accept as useful comparisons in IS where b and p are asumed to correspond between AAs and IE. (Beyond LL I have no opinion.) Consonant ablaut helps in sorting out such items as **b-l 'carry', with, inter alia, derivatives Aram. bar (**b-lH), Heb. ben (**b-Nl), Akk. ma:ru (**Nb-lH, with unexplained long vowel), all 'son'.

Many of IS's roots are not (yet?) represented in my base file, but those that are show that work on them must be brought to publishable form before an overall assessment of IS may be made. As things stand, I think that an entirely new set of Nostratic comparisons needs to be compiled (I think Bomhard would agree), using the consonant ablaut approach (of which I believe I am the only practitioner, though others may use the term). The IS material could be fed into this, but fresh data would be preferable. To me, IS is history.

Comments on Murtonen's comments. 6. My derivation is **b-w (negative) plus **l-C 'eye, see'. 8. **b-l 'carry' has widespread reflexes, e.g., **b-l Eg. n-b-3 'carrying-pole', Akk. waba:lu 'carry', **b-lH Ch. bare 'give', **bH-l Eg. f-3-? 'lift', **bH-lH IE *bher- 'carry'. 9. Probably from **b-l 'to swell, be(come) high', with affix. 10. The M reference should, I think, be to 8, which is correct. 12. AAs b does not correspond to IE p. 13. There is a base **b-l 'liquid, wet', but M's semantics deserve consideration. 16. I think of swelling = growing. 18. Probably 'brown' from 'burning' (**b-l 'to burn'). [There is considerable homoconsonantism among LL bases.] 19. See 16, with **b-l ~ **b-y. H an affix. 20. Same base as 13, as per M. 21. Probably same base **b-l 'to damage', as in 1. There is an **l-b 'to pierce' (discussion in press). 22, 23 N.C. 24. **b-l 'to burn', as in 18. Disagree with M. 25. (no M) Probably **b-k 'to bend'. **b-k yields both **b-q and *b-gh. 28. **b-g 'big', as in 17 (where the IE is from **bH-NkH). 29. **b-l 'swell' again (H an affix). 30. **b-d 'to bind'. **NdH yields IE *-ndh-. 31. Probably **b-w 'foot, place' plus **lH-k (from **l-k 'to go'). 32. Base **b-l 'carry' as in 8 (see Preface). The l is primary, contra M. 33. IS mixes **k-l 'cut' and **s-l 'cut', which must at this point be kept separate. While the existence of **s has been doubted (e.g., by Böhm), I continue to use it, at least ad interim. ... 42. S-n 'to know' is from (factitive) **s plus **l-C 'eye, see', i.e., **s-Nl-C. ... 60. M is right here. Base **d-(l) 'hand'.

One could go all the way through this way, but I would rather see a sizable and viable set of LL bases which are available for comparison and use. I'm working on it.

Yours,

Carleton T. Hodge

DEBATING THE ISSUES

MEMBERS RESPOND TO EDITORIAL ON RECONSTRUCTION.

        The first of what should prove to be many letters was from Igor
Diakonoff. There could hardly be a more experienced or authoritative voice
raised in this matter. We are honored by one of our founding fathers who is
probably soon to announce publication of a book or monograph on proto-AA.
After his letter, which he said I could have the pleasure of typing, the
others will be presented without comment -- except one. Paul Benedict has a
suggestion for collaboration between/among experts on Sino-Tibetan, Na-Dene,
and/or North Caucasic to work together on testing the Dene-Caucasic
hypothesis. We herewith invite Nikolaev, Starostin, Pejros, Pulleyblank and
others to contact Benedict or myself to explore possibilities.

Diakonoff's letter:

        " And now to my answers to your EDITORIAL ESSAY in MT5 which I
gratefully received yesterday [7 June - HF] (the postage mark was 6/4/89!).

        Question 1-2. I do not think there are national differences between
the different historical linguists but there does exist a difference between
people who use a bad technique and others who use a better technique.
        Question 3. Insufficient training in IE methods is obvious in some
of the work being done.
        Question 4. Differences among Indo-Europeanists. I think they are
unimportant. We can anyway not reconstruct the actual phonetics of pIE --
which, moreover, was not A LANGUAGE, but a dialect cluster -- and the
different solutions are a matter of description. I also think that the
problem of IE laryngeals can and will be solved when we get down to
Nostratic roots.
        Question 5. 'How many matchings (match ups) between two sounds are
needed before one can say that they correspond?' The formulation of the
question is unsatisfactory. Do you mean matchings between language A and
language B? Matchings of two isolated words between isolated languages are
worthless. You must show that these matchings are systematic, and that they
involve several languages, at least three. (For this [being systematic -
HF], you ought to have at least four or five correspondences in at least
three related branches of the family. This, unfortunately, is impossible in
the case of a few rare pAA phonemes.) And you must show that no alternative
matchings are attested or systematically possible. Borrowings can in
principle be sorted out by the fact that certain phonetic changes were no
longer productive at the time of the borrowing. Thus, Latin /caseus/
'cheese' is a borrowing because the characteristic change of *VsV > VrV was
no longer productive. The problem of sorting out the Northern French
(Normandy) borrowings in English is today no problem at all, it is so easy.
        Question 6a: How well should match ups or correspondences represent
a good SAMPLE of the phylum in question? The more examples we have, the more
sure we are of our ground. As to SN- // N-, the example of 'nose' in
Germanic is not unique: Indo-Europeanists have known about the prothetic S-
before sonants for a long time. The reason for the phenomenon has not, to my
knowledge, been explained, but the fact is there.
        Question 6b: Should one not at least take the internal structure of
a phylum into account when sampling? Sure. But on the other hand, note that
the internal structure of a phylum (or family) depends on the phonological
reconstructions. You certainly cannot take a word from Arabic and call it PS

[proto-Semitic - HF] and a word from Iraqw and call it PC [proto-Cushitic - HF] and take them both and call them pAA. But if your Arabic gloss is supported by other Semitic languages and the PS word is corroborated in several families of the AA phylum, you can use the solitary word from Iraqw, although it may not be as convincing as you wish.

Question 7a. Only quite obviously onomatopoeic words can be kept out of the comparisons. It is quite probable that many roots will prove to have been onomatopoeic in deep antiquity -- Gazor-Ginzberg even thinks that all words were originally onomatopoeic -- but once they are part of the vocabulary, they are subject to the common laws of phonetic change. A small number of onomatopoeic words are constantly renewed from children's speech. This answers also the question 7b. [Can't read - HF] /ts'ub-/ should not be regarded as onomatopoeic.

Question 8: A conclusion of the type '[a] in language X corresponds to [a] in language Y, so the phoneme was [a] in the protolanguage is, in itself, a NON SEQUITUR, because a phoneme in language X may correspond to several phonemes in the protolanguage (cf your examples concerning [s]). One must be especially careful with vowels, because we do not know A PRIORI what kind of influences may have been productive in the history of languages (apophony, stress, secondary noun-formative models, tones, etc.) Your question about [il-] and [ir-] 'eye' in Chadic etc. cannot be answered without systematic investigation.

Question 9 deals with comparisons of two languages only; under such conditions it cannot be answered, cf question 8.

Question 10: Semantic improbility. One should certainly keep the semantically improbable 'matches up' (Sirene des Gleichklangs!)** out of our linguistic reasonings. The great question, of course, What is semantically probable and what is not. I have studied primitive thinking not only on the base of linguistic data, but also on the base of mythology. The following can be regarded as a provisional rule-of-thumb:

A) Primitive thinking lacks abstractions. If you get an abstract notion as the supposed meaning of the word in the protolanguage, you have probably made a wrong conclusion. Do not look in the protolanguage for words with such semantics as {a round object}, {beauty}, {weapon}, {instrument}, etc.

B) Primitive thinking is metaphoric or metonymic.

C) Functional semantic connections are far more important than connections by similarity. You cannot connect a word for 'palm' in one language with 'cabbage' in another and 'onion' in a third on the ground that all three are green and all three are plants. But if you find that the same gloss means 'date' in one language and 'fig' in another, you can compare them, because both fruit had the same social role in the different societies, viz. that of supplying sweet in the diet. Or you can compare the name for 'henna' in one language with the name of a quite different plant in another, if the latter was also used for dying textiles. Und so weiter, as you say. **Your defense of the Sirene des Gleichklangs on p.35 is unwarranted. Antoine Meillet's dictum that, for linguists, not similarity but regular dissimilarity is important, is the base of all sound linguistic comparisons.

Of course there do exist exotic and seemingly improbable connections; but it is better.to leave out an actual connection which you cannot make plausible by the means you have at hand, than to indulge in disorderly comparisons.

Question 11a: The answer lies in your formulation of the question itself. By the way, one does classify Greek with Armenian (and to some extent with Indo-Iranian) [Does this difference among Indo-Europeanists make

any difference? - HF]

Question 11b: Does it make any difference whether the shared innovations (used for classification of languages inside a family or phylum) are lexical, phonetic, phonemic, or morphological? It does. Phonetic innovations are unimportant (anyway we do not and cannot reconstruct the phones of a protolanguage) until they reach a phonemic status. What do you mean by lexical innovations? Borrowings? They must be kept out. Semantic changes? They are not really lexical innovations, because the same old roots are used. -- As to morphology, the rules of morphological change is a problem apart. Ideally, we should have linguists working on phonological reconstruction of words, and linguists working on historical morphology.

You must have taken the theory of the development of the French [R] because of the defects of speech characteristic of Louis XIV from some source. But I don't believe the theory is sound. Such cases are known in certain minor linguistic groups descending from one family -- but a whole nation? As to Germans, their -R in Auslaut is more like an 9ain and not at all like the French [R]. So this is not a shared innovation. [Methinks you underestimate the power of royalty - HF]

Question 12: The subclassification is a result of linguistic reconstruction. You cannot use it FOR linguistic reconstruction. However, once you know the subclassification, you must take care you do not contradict yourself in your reconstructions.

Question 13: How does one know that you can segment the lexeme BRIGHT into BRI- and GT? Answer: you cannot segment it unless you are sure that a morpheme {gt} existed, and unless you are sure that the root morpheme {bri-} is of a pattern admitted for root morphemes in that particular language or family. S- in 'nose' is not an 'undetected morpheme'.

Question 14: critique of the Indo-Europeanists' methods. Cf my note to your question 10.

Question 15a: Your list of morphemic (!) correspondences between Chadic, Cushitic, Omotic, NE Caucasian, Khoisan and Altaic is beside the point. One cannot compare morphemes just as if they were phonemic parts of the roots in question. The development of morphemes is a case quite apart. [There must be a cognitive chasm between us because I cannot understand what the criticism is here - HF]

Question 15b: Shall we demand that people stop ignoring data or should we shrug it off as inevitable? Data should not be ignored. But some data may be irrelevant -- not in the examples you quote, however.

Question 16a: The resurrection of the protolanguage should not be a goal in itself. It must be used (a) for reconstruction of the natural and social milieu of the bearers of the protolanguage, (b) for further (deeper) linguistic reconstruction. This, to my mind, answers also your Question 16b. By the way, the number of Assyriologists in the world is by far not sufficient to publish and to make sense of the available texts.

Question 17: I don't know anything about Amerindian, but I am entirely in agreement with S.J.Gould.

And that is that. Thank you for attention.

Yours truly,

Igor Diakonoff

June 17, 1989

Dear Hal,

It was a great pleasure, as always, to look over the latest
issue of MT. I see that you are still in fine fettle, and I esp.
appreciated your contribution on methods of recon. You asked for
some comments and here are some of my thoughts.

5. one match up makes a correspondence, esp. if it fits within
an over-all framework, e.g. one might have several for /p/ and /p/,
/t/ and /t/, etc. but perhaps only one for /q/ and /q/. If instead
of the last, the one likely match up indicates /q/ and /zero/ we'd
all like to see another example, no? (I'm assuming a /q/ is present).

6. The point here is that this Germanic-attested 'nose' root
certainly cannot be set up for PIE but neither can it be excluded!
This point appears often to be overlooked. In ST we work with one
language, basically, Chinese on one side of the fence as opposed
to 200 or more on the other, TB side, and at times it can be shown
that only one language/group has preserved a PST-level, hence also
PTB-level, root, e.g. FACE, with a PTB-level *s-mel (the same
body-part *s- prefix as in *s-na underlying 'nose' - see under 13)
maintained only in Kuki-Naga (Lushai hmel) as shown by the perfect
Chinese cognate: Archaic mian or prob. *s-mian (the archaic Min
dialects reflect the *s-), with -ian < -el â regular shift. This
is a matter of vast importance for LR workers since it means that
one can indeed make use of a root represented in only one language/
group of a family or stock, altho one must grant that a well-repre-
sented root is to be preferred. Incidentally, I've now adopted the
ff. convention: a root broadly enough represented to be reconstruct-
able at the proto-level, even if at only two extreme points, as
in the above root for 'face', is preceded by the plain abbrev., e.g.
PST *s-mel 'face' whereas a root without this kind of represen-
taion (not reconstructable at the proto-level) is preceded by the
abbrev. plus hyphen, e.g. PTB- *s-mel 'face'; note how this root
is establishable at the PTB level only because of its representa-
tion in Chinese. (but PTB- used since no other TB representation).

10. Blust's Diachronica paper should be required reading for
all comparativists. Blust has an anthropological background (MA)
and it shows. I've long argued that all comparativists should
begin as anthropologists but I could be biased. It so happens that
Blust's paper also furnishes a magnificent example of the above!
He presents at some length several roots for 'house', both of the
PAN and PAN- varieties (see how useful my convention can be!), but
the most likely candidate for establishment at the earlier PAT level
is none of these! It is, rather, a root represented in AN only
in the (often archaic) Rukai group of Formosan: [d,dz]ayan, which
corresponds perfectly both with Kadai *raan (regular /r/ for *y along
with 'vocalic transfer') and with Japanese ya (regular shifts)!
This should serve as an object lesson for all LRers!

initial ∧

13. This also a big problem in AN - see Blust's recent work on AN roots, which factors out the equivalents of bri-/, in this case lopping off elements and ending rather with the likes of -ot. Significantly, these 'roots' (Blust admits that they are unlike roots as we normally think of them) have very few ties to the mainland families of AT: Kadai and Miao-Yao, or to Japanese, rarely involving Swadesh-list items (as noted also by Blust). It is clear that the vast bulk of roots in AT are disyllabic, much as those in ST are monosyllabic. As for the ST 'nose' form, I've now shown (forthcoming, based on talk at ST conf. in Vancouver, 1988) that the PST *(s-)na root basically meant 'headwaters', later applied (only in TB) to 'nose' as 'snot headwaters', also that PTB *s-nap 'snot' basically meant 'sthg. soft';neither root had any basic connection with 'nose' or the like and if one wants to cite a TB or ST form here he has only PTB *(-)nam 'smell' (*s-nam as tr. vs. *m-nam as intr.), with both *-num and *-nim variants in the picture (but none app. represented in Chinese).

*has only *s-na eg.*
= 'marsh'
(← 'head-
waters')

15. The dishonesty comes about when one is aware of the above but appears to suffer a loss of memory (like some of our Washington politicians on the stand) and goes on citing, in support of a hypothetical underlying *SN- root, that beautiful trio: PTB *s-na 'nose', *s-nap 'snot', *s-nam 'smell', 'forgetting' also that *s- is a PST-level body-part prefix as well as transitivizing verbal prefix. True, one doesn't have to take some other (in this case, my) scholar's word for it but if he disagrees it is incumbent upon him to explain just why he wants to change it all, e.g. the *s- is not a prefix in these forms and/or my analysis is all wrong for the ff. reasons. A minor form of dishonesty occurs when a whole series of forms is cited, e.g. 20 or more TB forms for 'four', instead of the PTB *b-ləy cited in my Conspectus, which the author in question has made use of, it is clear. Worse, this mélange of forms is often cited instead of the root! This sort of thing qualifies not only as dishonest, after a fashion, but also as stupid. Again, one surely is free to disagree with a given recon. but in that event is obliged to give his reasons, no?

Enough for now. I'd like to see that Sino-Caucasian thesis worked on, as I described in earlier letter. Can't you get someone to do the Caucasian side, someone who has the Caucasian files (see p. 35) available - and who knows the phylum. I'll do the ST, of course, and if you want someone else probably Matisoff would be willing to serve (he was Contrib. Ed. of Conspectus). I still feel that a 'test run' must be made, ideally under your supervision, and S-C should do nicely in view of all the recon's already at hand on either side of the fence (as you note, this often not the case). Izzy's letter was no surprise - even polite, for him! But then, he's not an anthropologist so what can one expect?

I'm now working on revisions of both my ST and my AT stuff, with a view to getting out up-to-date glossaries of roots for both. I'll keep you informed.

Best

Paul

M. L. Bender
401 Emerald Lane
Carbondale, Ill. 62901

Some of the roof
decorations
in the Church of
Ende Abune Aregawi,
Debre Damo,
Ethiopia.

Dear Hal,

Although I was one of the first supporters of your newsletter,
which has evolved (so far) into "Mother Tongue", I have not written
you anything for publication. I am now doing so (does this qualify as
a "performative speech act"?).

I am glad to see that you are taking my advice,which from the
start was to turn the newsletter into a refereed journal so that at
least a part of the scholarly community will take it seriously. It
seems that it needed a lot of other voices added to my own to bring
this about- but in this case, perhaps the consensus should prevail
(unlike with issues such as the validity of proto-forms for which we
prabably do not want to take votes and declare the attainers of major-
ities the "correct ones"!).

There is no unanimity among your critics as seen by the differ-
ing views of such renowned scholars as Diakonov and Lehmann, among
orhers in recent issues, despite the seeming existence of a "Harvard-
Smithsonian" axis. I am content to include myself among the friendly
critics who may keep some of the -frankly speaking- crackpots who are
attracted to such a stimulating enterprise under reign. We should wel-
come diverse opinions from within our own ranks.

For example, I think there has been too much "talking past one
another" on the "similarity" question. Right now, in my Central Sudanic
comparisons, I came across the item mbata for "bed" in Kresh, Aja,
Sara-Mbay, and "Jur-Beli". I suspect this is a loan from somewhere to
all of these (can anyone identify it- maybe English?).Why? Because it
is too similar in all four cases! I like much better for comparative
purposes things which show some dissimilarities- especially if they
fit a pattern I've already noticed in the same languages. Isn't this
what the critics are saying? Are they really saying that one should
disregard similarities altogether? I don't believe so.

At the Michigan meeting I presented a paper in which I tried to
test the role of chance when one undertakes "global etymologies". The
revised version I sent to Vitaly for publication is much different- and
I hope, much improved. Recently I had an exchange of correspondence
with John Bengtson about a purported mathematical argument in support
of "global etymologies". I think the argument does not support g. e.'s
at all, and I still think you guys are wasting a lot of time piling up
more and more of these "similarities" whose significance is a mixed
bag of chance, diffusion, universals, and even some genetic relation-
ships. All those 17-plus intersting and difficult questions you ask in
the last MT should make you realize that it doesn't make sense to go
about picking out similarities among all world language phyla in such
a superficial way.

Not that I'm against "Nostraticisms": contrary to most of them, I think Afrasian belongs to a Eurasian phylum, but I do not question the wisdom nor even the methods of those working in these inter-phylum comparisons. I am not qualified to say "Sino-Caucasian" makes sense or that the case is settled for "Amerindian", but I do think this is the direction we should be going- plus microscopic work in such areas as Central Sudanic and Nilo-Saharan more generally- not "world etymologies".

Regarding Starostin's "new glottochronology": what he presented at Michigan is, in my humble opinion, worse than the old glottochronology (which I used to do- and still do in a very limited sense as a heuristic and relative-chronology device). Theoretically, it makes little sense, and practically, it is a nightmare unless one has unlimited research-assistant support.

Finally, a word regarding your pessimism about Omotic.
I don't know how one field trip you are planning to Ethiopia will add that much. For one thing, persons already at work there have added to the data base considerably (of course we don't have access to all of it yet), For another, I have already done a preliminary Omotic lexical-segmental phonology reconstruction (copy sent to you recently) and have two other versions in press. Finally, knowing the present unsettled conditions in Ethiopia, you may not be able to get out to the areas where the greatest data gaps are and it will be tough to find speakers of most of these rare varieties in Addis Ababa. Similarly, in Nilo-Saharan, a lot of good work is now being done in selected families (notably Nubian and Nilotic) and I expect much more from the Bayreuth meeting in Aug.-Sep. than you do from those of us who   are still in the trenches working on micro-projects while keeping our eye on the larger picture as well.

Sorry, it is time to quit when my eloquence dries up and I start nitpicking. Keep it up- you are a true pioneer, and MT can become an important journal of an important organization if we face up to the tough task of organization-building.

Lionel

ANNOUNCEMENTS

1.   4th NILO-SAHARAN LINGUISTICS COLLOQUIUM in Bayreuth. The very
important topic of Nilo-Saharan and its deeply divided branches and
difficult reconstruction is available this summer (August 30-Sept.2, 1989)
in Bayreuth, FRG. A number of good papers are scheduled, especially those
that probe the taxonomic positions of Songhai and Saharan, as well as the
Kadugli problem where a group of languages previously classified as N-C are
now treated as N-S. Some readers misunderstood my statement that "it could
be argued that the greatest benefit obtainable would be from the
conversations over refreshments and discussions after papers. This is
because most of the world's N-S scholars will be there and none of them can
give more than a smidgen of her knowledge in ordinary conference paper
time." I meant to stress the great value of talking with this group of
specialists, not to denigrate the quality of the papers -- for heaven's
sake! It is a great opportunity for talking about things and picking
people's brains. Anything run by Franz Rottland is likely to be warm and
friendly in atmosphere, so good communication is likely. For more details
write to Professor Franz Rottland, Lehrstuhl AFRIKANISTIK II, Universitat
Bayreuth, Postfach 10 12 51, 8580 Bayreuth, FRG.

2.   CUSHITIC AND OMOTIC CONFERENCE IN TORINO. Under the inspired
direction of Giorgio Banti a conference on Cushitic and Omotic languages and
histories was arranged for Turin, Italy, for early summer. Due to the large
response and the formidable logistics of a big international conference,
Giorgio and his colleagues moved the date to around November 1, 1989. There
will be many very good papers and the added treat for Westerners to meet the
Soviet delegation (Militariev, Belova, Aihenvald, Vetoshkina, Porkhomovsky,
et al) whose prowess at Afrasian linguistics has not yet been fully realized
by non-Europeans. For more information write Professor Giorgio Banti,
Universita' di Roma "La Sapienza", Dipartimento di Studi Glottoantrologici,
Piazzale Aldo Moro 5, 00185 Roma, Italy.

3. What was to have been a conference on the INDO-EUROPEAN SUB-STRATUM, to
be held in Yugoslavia, has been transformed into a conference on THE
TRANSFORMATION OF EUROPE. It will be held in Dublin, Ireland, at University
College Dublin in September. Participants will include linguists,
mythologists, archeologists, and physical anthropologists. It is anticipated
that a number of papers critical of Colin Renfrew's hypotheses will be
presented. One may write to Professor Maria Gimbutas, % Karleen Jones Bley,
2143 Kelton, West Los Angeles, California, 90025.

4. A conference which has now ended would be a good one to check up on. It
about transcription and the IPA and a discussion of the revision of the IPA
and related matters like a standard ASCII code for phonetic symbols. All
this from our good colleague, Kay Williamson, whose letter unfortunately
consumed FIVE months in getting from Nigeria to Massachusetts! For more
information about the conference, and more particularly the results, write

to Caroline Henton, Linguistics Program, University of California, Davis, CA 95616, U.S.A.

5. Robert Blust has had published his book AUSTRONESIAN ROOT THEORY: AN ESSAY ON THE LIMITS OF MORPHOLOGY. By John Benjamins. Studies in Language Companion Series, 19. Amsterdam/Philadelphia, 1988, 190 pages. According to the blurb, "Analysis of the Austronesian root suggests that there is a level of language structure intermediate between the phoneme and the morpheme, which has different surface realizations in different languages or language families." Paul Benedict and I are both excited about the book which is positively on target for our discussions of segmentation among other things. It will be reviewed in MOTHER TONGUE.

6. Jan Wind, Edward G. Pulleyblank, Eric de Grolier, and Bernard H. Bichakjian are the editors of STUDIES IN LANGUAGE ORIGINS, VOLUME 1. Also produced by John Benjamins, Amsterdam/Philadelphia, 1989, 331pages. The editors are the heaviest hitters inside the book which contains some very useful and thought-provoking articles. It will also be reviewed in MOTHER TONGUE.

7. John Bendor-Samuel's book on Niger-Congo, which we high-lighted in MT-6, has now come out. The book remains exciting to me because of the several important changes in internal taxonomy about which a number of scholars are in agreement. The taxonomic appellation NIGER-KORDOFANIAN is now moribund because Kordofanian was been downgraded from a coordinate half of the great phylum to one of three equal branches -- Mande, Atlantic-Congo, and Kordofanian. It occurs that a Niger-Congoist might be the best person to review Robert Blust's book.

8. According to Kay Williamson, Ben Elugbe's forthcoming book COMPARATIVE EDOID is "an important detailed reconstruction of a New Benue-Congo group which should be of interest to all working on Niger-Congo reconstruction. The price is not yet fixed, but it will not be very expensive (in dollars) and advance orders will help us to finish paying the printers." That sounds like a bargain to me!

9. We neglected to mention that Merritt Ruhlen shares with Paul Benedict a desire to work with several other people on building etymologies through using experts on various families and phyla so that poor cognates and known borrowings and such can be swiftly eliminated. The results of such common efforts, as for example the attempt to test Dene-Caucasic, may be published in MOTHER TONGUE when she becomes a proper journal.
....

COUNCIL OF FELLOWS.

     Although the Board of Directors have done nothing formal as yet about this idea, we are soliciting nominations for the Council. The point of the Council will probably be to honor some people for outstanding work in the general area of language in prehistory. So, if you want to nominate someone to the Council, including yourself, please write to the Secretary and tell her of your choice.